



Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

Book Reviews

"THE HISTORIC EXODUS"

A REPLY TO A REVIEW BY DR. D. D. LUCKENBILL

In the *Biblical World* for December, 1909, there appeared a review of my book, *The Historic Exodus*. The nature of the review was such that a reply is demanded. It is not the usual custom for a writer to respond to the reviews of his books, but in this case the reviewer has so attacked me that I think silence would be but a confession of weakness, and that a reply is demanded for self-protection. I wish to show both how grossly Dr. Luckenbill has misinterpreted my position, and how grievously he has gone wrong in stating what he considers to be the facts in regard to that position.

1. While discussing my statement of the modern higher critical position, the reviewer calls me to account for ascribing the origin of that hypothesis to Wellhausen, saying, "Any beginner in the study of Old Testament criticism should know that Wellhausen was not the originator of this hypothesis." Never in my book have I said that he was. What I did say was that to him belonged the "distinction of having pointed out that the Hexateuch is made up of *four great documents*." That does not imply that no one ever imagined such a thing as a four-great-document hypothesis before Wellhausen. It does state, what is eminently true, that he was the one to "point it out"—to bring it to the attention of scholars in such a way that it became a prevalent mode of thought. And this, it seems to me, is quite plain when we remember that the hypothesis is known as the "Wellhausen Hypothesis" the world over.

2. In one place in my book I stated that "the use of the word 'document' presupposes that we deal here with a complete document and not with mere fragments." The reviewer objects that by this sentence I attach a meaning to the word "document" which it has never had in criticism. It will doubtless prove interesting information that the word "document" in criticism means something other than when used in any other department of language. It is my impression that the documentary hypothesis succeeded the supplementary hypothesis, which in turn succeeded the fragmentary hypothesis, which in turn succeeded an older documentary hypothesis. Would the reviewer have us to believe that the documentary hypothesis is merely another name for the preceding fragmentary hypothesis?

One of the contentions in my book was that the latter of these is more nearly fitted to the facts than the former. If the two are one, then indeed on this point I do not differ from the extreme advocates of the documentary theory. But I doubt whether many of them would accept the reviewer's definition of their position.

3. The review then proceeds to charge me with the use of material of other scholars without giving them proper credit. It is alleged that I have borrowed most of chaps. ii and iii from Professor Eerdmans' work, *Die Komposition der Genesis*. Inasmuch as it is hardly possible to charge me with a more unscholarly and ungentlemanly act, I feel called upon to answer this rather fully. The reviewer states himself that in my preface (p. xi) I acknowledged that in these chapters I agreed in many details with Eerdmans. In another place in that preface (p. xi) I stated that I refrained from cumbering my pages with footnotes to the works of scholars from whom I got some of my ideas, and I gave the reason therefor. In doing this I was fully in accord with the example of many distinguished scholars. Singularly enough, Eerdmans, in the very work from which the reviewer says I took my ideas without giving proper credit, adopts this same method, although it is quite apparent that he didn't evolve all his ideas out of his inner consciousness, but was willing to depend somewhat upon the accomplished results of his brother scholars. Further than this, on the same page, I declared that in the appended bibliography I had given a list of works which I had freely used in writing the volume. And in this appended bibliography, among a good many other books, is the book of Eerdmans mentioned above. If the reviewer had wished, he could have found a good many other instances in which my ideas were the same as other men's, and where I did not give specific credit.

4. But the particular examples of this alleged fault of mine deserve some little attention. The reviewer believes that I derived my argument concerning the absence of the Persians from the list of nations in Genesis, chap. 10 (P document), from Professor Eerdmans' book. But long, long before Eerdmans many scholars had noted this difficulty and had tried in various ways to account for it, because they felt the necessity of believing that P was written in the Persian period. It is true that Professor Eerdmans, in writing on this subject (p. 9), cites no previous authorities for his argument; and the reviewer has doubtless supposed that it originated with him.

5. In regard to the argument from the mention of the Elamites among the children of Shem in Genesis, chap. 10, which the reviewer says I took in a similar manner from Eerdmans, it may be sufficient to state that my

argument therefrom and Eerdmans' argument therefrom are entirely different. The only identity is in the statement that the Jews of Ezra's time knew that the Elamites were not Semites, a statement of fact notoriously true, which would hardly be stated by two people in ways differing very radically from one another. Orr stated the same fact two years before Eerdmans (cf. *Problem of the Old Testament*, p. 401), and Driver and Hommel noticed it long before Orr.

6. But even supposing that the reviewer's comparisons were more conclusive, has he failed to notice that there are many arguments in Eerdmans which I cannot possibly be accused of adopting, and that there are many, many arguments in these chapters of mine, and in my other chapters on J, E, and D, which are not even suggestive of him? Even were it true that the six points of the identification were really established, is it quite fair to insinuate that as a result the whole of my consideration of higher critical dates is taken, practically *in toto*, from Eerdmans' work? This is a misrepresentation that is simply exasperating. As a matter of fact, the major portion of my argument for the statement that P is not post-exilic is based on three things; (a) the tracing of the institutions mentioned in P as far back as the time of Saul; (b) the evidence of the presence of P in early times; and (c) the evidence of the use of the "Law of Yahweh," the P law, in very early times. In these points I am, so far as I know, on grounds quite untouched hitherto by any writer on the subject, and it is on these points that my argument in the main rests. None of these important points has received the slightest attention at the reviewer's hands.

7. I am supposed by the reviewer to have derived my treatment of the Toledoth Book from Eerdmans' work, despite the fact that fully half the material assigned by me to the Toledoth Book is not so credited by Eerdmans, and the fact that he regards it merely as pre-exilic, while I declare it contemporaneous with the Exodus of JED. Eerdmans and I, it is true, both treat of a Toledoth Book. So does nearly every other scholar who writes on the subject.

This much time has been taken with this charge, not because of its intrinsic importance. It has none, in the eyes of those who have read both Eerdmans and myself. But probably most of the readers of the *Biblical World* have not read both works, and to them the charge may possibly have seemed to have been founded on facts.

8. Passing from the accusation in regard to my originality, I go on to the reviewer's remarks regarding my philology. I ventured in one place (in a footnote, entirely unconnected with the argument) to trace an interest-

ing connection in regard to El-Shaddai. I suggested that El-Shaddai was the same as the Egyptian god Sed or Set, the center of whose worship was Tanis and Avaris. For this identification the reviewer says quite confidently that there "is not a scrap of evidence." What he esteems evidence in such cases, I do not know. Apparently the fact that the names are philologically identical, the fact that they are both primarily phallic gods, the well-known fact that Sed or Set was identified with the Hyksos god Sutekh, which god the Hyksos brought with them from Syria, are of no importance whatever. This god, as I went on to state, has the totem of an ass. Since he is Syrian, what is more natural than that perhaps he is to be identified with Hadad, the god of the Amorites? I point out then that the Amorites probably took their ethnic name from this ass symbol, since the name "Amurru"¹ probably means ass, being connected with *Chamor*, meaning "he-ass." This the reviewer apparently does not understand, and seems to accuse me of a wonderful philological identification of Sed or Set=Saddai=El-Shaddai=Hadad of Amurru=Chamor.

9. The next philological point criticized is in regard to my statement that the Canaanite tongue mentioned in Isa. 19:18 was Galilean Aramaic. The reference in Isaiah is, of course, to the Hebrew worship established in his time in five cities of Egypt. The reviewer tersely asks, "Where is the evidence?" In reply I would say that it is usually safe to assume that some axiomatic facts are known among scholars. To those who have read on this subject it is well known: (a) that inscriptions found at Zinjirli, in northern Syria, dating from the middle of the eighth century, show that the Aramaic peoples were well established in *northern* Palestine at this time; (b) that the Zakar inscription, dealing with Galilean matters, is written in an Aramaic dialect; (c) that in the time of Sheshonk I (*ca.* 935) a place was conquered, whose name, as W. Max Müller has pointed out, is undoubtedly Aramaic, called "The Field of Abram," and that this place was in *southern* Palestine, in Judaea. Aramaic, then, was by no means impossible *throughout* Palestine in the time of Isaiah. Further, it is well known (d) that all the inscriptions so far discovered of the Jews that dwelt in Egypt in early times are not in Hebrew, but in Aramaic, very like that of the Galilean inscription of Zakar, mentioned above. The time is now past when any scholar can interpret the word "Canaanite" in Isa. 19:18 as meaning "Hebrew," or anything else than Aramaic. Now we know that the great reason for extensive migration of Jews into Egypt previous to Isaiah was the conquest of Naphtali and Zebulon, Galilean tribes, in 738

¹ I take "Amurru" to be the Canaanitic equivalent of Assyrian *Imêru*, "ass," which was an integral part of the old name of Damascus, the capital of the Amorites.

B. C. And this, in connection with the fact that the Aramaic of the Egyptian inscriptions is quite different from that which we know was later spoken in Judaea, I think is quite sufficient justification for terming the tongue in question "*Galilean Aramaic*."

10. In another part of the book the reviewer found a reference to a Galilean dialect, in which I stated that the Song of Deborah, the Song of Songs, and the Book of Jonah were written. He jumped to the conclusion, entirely unwarranted by anything in the book, that this Galilean dialect was "*Galilean Aramaic*" too. And this in spite of the explicit statement, given in the very next sentence of my book, that this dialect did not contain Aramaisms at all. Indeed, the reviewer quotes this denial, and then accuses me of inconsistency. Is it utterly impossible to believe that there was in the time of Isaiah a dialect of Aramaic spoken in certain Galilean tribes, and that there was also, at all times a Galilean dialect of *Hebrew* spoken in the same land? The reviewer has utterly failed to grasp the fact that the Hebrew tribes were not at this time one united nation, unified in language, but a conglomerate of more or less unrelated tribes, with a multitude of warring interests and differing speech.

11. The reviewer then proceeds to make fun of my general treatment of the Hebrew dialects, and seems to doubt the possibility of there having been such things. I said that *bosheth* in Benjamin is the same as *baal* in Jerusalem. This identification, being foreign to the usual critical idea on the subject, seems foolish to the reviewer. As it happens, I am not alone on this. After my book was written, as I was reading final proof, I received Böhl's *Sprache der Amarnabriefe*, published in "*Leipziger Semitistische Studien*." On p. 5 this author also says that the difference between the words is due to dialectic peculiarity, not to a difference of meaning. So far as the reviewer knew, I differed from other scholars. Ergo, I was wrong.

12. Then he says, after his statement of my position in regard to *bosheth* and *baal*, "This thought is developed until on p. 138 we have P written in the dialect of Levi, E and D in the dialect of Ephraim, and J in the dialect of Judah. Where is the evidence of all this?" Now, if this means anything, it means that I based my assigning of the documents to tribal dialects entirely on the difference between *bosheth* and *baal*. Of course, I did nothing of the kind. As for the demand for evidence, on pp. 139, 140, some rather significant philological evidence *was* given. The recognition of Hebrew dialectic peculiarities is no new thing among those who keep abreast of the times, and are not satisfied with blind loyalty to hypotheses as yet unproved. Dr. Driver, for instance (*Introduction to the Literature*

of the Old Testament, 12th ed., p. 449), has pointed out that in the post-exilic and late writings these dialectic peculiarities are present, and that there is evidence of them even in the early Song of Deborah. I have simply carried the investigation into the documents of the Hexateuch. The reviewer rightly catches the point of the contention that *if the peculiarities of the documents are due to dialectic and not developmental causes, there is no criterion in their language for dating them*. This was new. The reviewer has not found it in his critical authorities heretofore. Consequently it must be discredited, even at the cost of unfairly stating what the position really is.

13. The reviewer thinks he has caught me in an inconsistency when I say, in one place, that the peculiarities of language in the various documents were due to dialectic differences and not chronological development, while I stated in another place that the Toledoth Book could be distinguished from P proper because its language was earlier. He has evidently failed to understand me. I based my separation of the Hebrew literature into dialects on the basis of differences of *vocabulary and phrases*. But within each dialect, thus distinguished, I found various stages of progress, and disintegration, not in the vocabulary, but in *grammar and syntax*. There is no reason, on this method of division, why P, J, E, and D should not be of different dialects, while in each there is a series of progressions, grammatic and syntactic. I should think that this would be quite plain to one who had read my book carefully, especially the chapter called "The Language of P."

14. Then the reviewer proceeds still further in his attempt to discredit my philology. He seems to doubt, for instance, my statement that "Tharu" is the exact equivalent of "Shur." As he states it, I am made to say that "Tharu is the exact *Hebrew* equivalent of Shur." This is nonsense, of course. "Tharu" isn't the Hebrew equivalent of anything. It is *Egyptian*. What I really did say was that the *Egyptian* "Tharu" is the exact equivalent of the Hebrew "Shur." If the reviewer doubts this identification, let him read W. Max Müller, *Asien und Europa*, p. 102, or Hastings' *Bible Dictionary*, where Professor Müller's argument is cited in the article on "Shur."

15. The reviewer doubts also that "Khetem" equals "Etham." In a footnote on the very page where this statement is made, I have given as my authority for it Brugsch, *Dict. Geog.*, p. 647. I am inclined to admire the reviewer for airily disagreeing with Heinrich Brugsch.

16. He finds what he calls his "choicest specimen" of my philology in my remarks about the city *Tu-mur-(ka)*, p. 264. I claimed that *tumur*

is the Arabic plural of *tamar*, meaning palm tree; and I therefore felt inclined to identify the city with the biblical Jericho, also known as "The city of palm trees." Now the inscription wherein this *Tu-mur-(ka)* is found dates from about the year 1400 B.C. The reviewer declares my identification impossible, because I thus imply that there was in use "an Arabic plural of palm tree at least a thousand years before the Arabs could possibly have pushed into Canaan!" Does he really believe that the Arabs did not come into Canaan until about 400 B. C.? Does he know nothing of the investigations of Dr. Glaser, not to speak of Hommel, Müller of Vienna, Halévy, Sayce, Nielsen, and others? There have been brought to light over one thousand inscriptions, written in old Arabic, from South-Arabia, known as the Minean and Sabeian inscriptions, many of them dating from the second millennium B. C. The Sabeian kingdom was at its height about 950 B. C., as we know because its queen visited Solomon. It must have been old at this time. And it had displaced the older Minean power. The latter, called Maon in the Bible, *had colonies in Palestine*, as we know from the Bible and the inscriptions, long before 1400 B. C., one east of the Jordan, the other just south of Judaea, both in the older territory of Benjamin. And yet, in the face of facts as well known among scholars as these are, we are told that Arabic plurals in the Amarna letters are impossible, or anywhere in Palestine before 400 B. C.!

17. My treatment of *Cus-Arsathaim* (Hebrew text *Cushan-Rishathaim*) is next taken up. I stated that Artatama, according to the Boghaz-köi inscriptions, came from *Ku-us-(sar)*. I then ventured to identify the two men and to say that *Cus* is probably a place-name and identical with *Ku-us-(sar)*. The reviewer ridicules this idea, asking if I can drop an *r* (*sar*) at will. Not at all. But has he thought of the possibility of the *r* in this case not being a portion of the word stem at all? May it not have been a case-ending? In our present knowledge of Hittite, from which land the inscription was written, and by which it was undoubtedly influenced, it is sheer presumption to assume that the *r* is necessarily an integral part of the stem.

18. Having attempted to establish the fact that I am no philologist, the reviewer seeks to discredit my historical and geographical ability. "As to the treatment of the historical data," he says, "it is safe to say that there is no scholar of any standing in the scientific world who could possibly agree with the distorted and positively misleading interpretation of the Egyptian and Babylonian monuments which is here presented." I have in my possession private letters from a number of the leading scholars of Europe on Assyriology, Egyptology, and the Bible, written since my book appeared, complimenting me particularly on this very point, my

interpretation of the Egyptian and Babylonian monuments. Even critics who have felt inclined not to accept my theory *in toto* have agreed that my interpretations are scientific and plausible.

19. Then follows an equally sweeping denunciation of my geographical ability. The reviewer gives but one example of my ignorance in this line, saying that I reach "the height of the ridiculous" when I state on p. 176 that "the Wilderness of Sin and the Wilderness of Sinai are the oases of the Arabah, between the Seir ranges," etc. By italicising the word "oases" and putting an exclamation point after the same, he makes it plain that what he considers ridiculous about this statement is that I call a "wilderness" an "oasis." An oasis, according to Webster's *Dictionary*, is "a fertile or green spot in a waste or desert." The word in Hebrew which is translated "wilderness" is *midbar*, which, according to Brown, Driver, and Briggs's Hebrew Lexicon, in its first or common meaning signifies "tracts of land used for the pasturage of flocks and herds." And if the reviewer thinks there are no such "oases" or "wildernesses" in the Arabah, between the Seir ranges, etc., I would respectfully refer him to Stanley, *Sinai and Palestine*, p. 161, where that distinguished and accurate traveler defines the vicinity of Petra as "an oasis of vegetation in the desert hills."

20. I now come to a consideration of the reviewer's conclusions. "The whole method of the book is totally unreliable," he says. An examination of the internal evidence in the documents themselves, and a reconstruction of Hebrew history according to the requirements of the evidence—that is the method. If that is totally unreliable, so is all the critical study of the past generation. Having made this categorical announcement, the reviewer, backing it up, gives what purports to be a short synopsis of the book, which differs utterly from what is to be found in it. The first part of the book is, as he says, devoted to a reconsideration of the dates of the documents, and the result of this is, as he also states, that I conclude that they were not late, but early. The latter portion of the book is devoted to my reconstruction of Hebrew history of the Exodus period, which results in the belief that there were two exodi, some hundreds of years apart, the accounts of which in latter centuries become fused. Thus far the reviewer is fairly on the track of my position. But then he says, "The proof presented"—for this double-exodus theory, of course—"consists largely of linguistic arguments which we have already discussed." This is contrary to fact. Out of 339 pages in the book, only 10 pages, and a few notes, are devoted to linguistic arguments. The double-exodus theory, as a matter of fact, is based upon the tremendous differences to be found existing between P on the one hand and JED on the other, as to the settlements in Egypt,

the routes of the exodus, the length of the wandering, the mountains Horeb and Sinai, the sets of tables and the arks, the legislations of Horeb and Sinai, and the priesthoods. In other words, the theory is the result of examination of the documents, each one entirely by itself, and a comparison of the internal evidence deduced from each of them. None of this, which constitutes fully half the book, is so much as hinted at in the review. The reviewer has picked out minor points here and there, philological and otherwise, often from the notes, has sought to controvert them, and has then claimed that he has discredited the whole book! The argument as a whole, the significance of the new way of looking at the Exodus—the point of the whole book—has been utterly ignored.

21. In his last paragraph, the reviewer quotes my statement (p. 279), that the Hexateuchal documents are historically accurate *except where redactors at later times have made changes*. He says that the exception nullifies all the contentions which I have advanced. It is simply a fact that I state. Everyone knows that there are certain passages in the Old Testament which have received redactorial glosses. And yet my theory is completely vitiated by the admission. If mine is, so is that of every modern scholar on this subject. I know of none who denies that later copyists have in places annotated.

22. I now come to the last sentence in the review: "There is no immediate danger that the hypothesis here presented will necessitate 'a total reconstruction of the Evolutionary Hypothesis of modern higher criticism (p. xii)'." I did not say this. What I did say was that "the *acceptance* of my hypothesis would necessitate such a reconstruction." But I waive this point. My hypothesis is that the documents, by their own integral evidence, are early, not late; that P agrees with the history of one period, and JED with that of another entirely different; and that therefore there is no reason to suppose the documents unhistorical, and every reason to suppose them accurate, when assigned to their proper historical backgrounds. Such a theory, backed up by facts and arguments, most of which the reviewer does not notice, and none of which he disproves, may indeed, *unless it be answered*, render a reconstruction necessary.

OLAF A. TOFFTEEN

WESTERN THEOLOGICAL SEMINARY
CHICAGO, ILL.

A REJOINDER

1, 2. The reviewer stated that critical scholars recognized no such definition of the word "document" as Professor Toffteen gives it. At the